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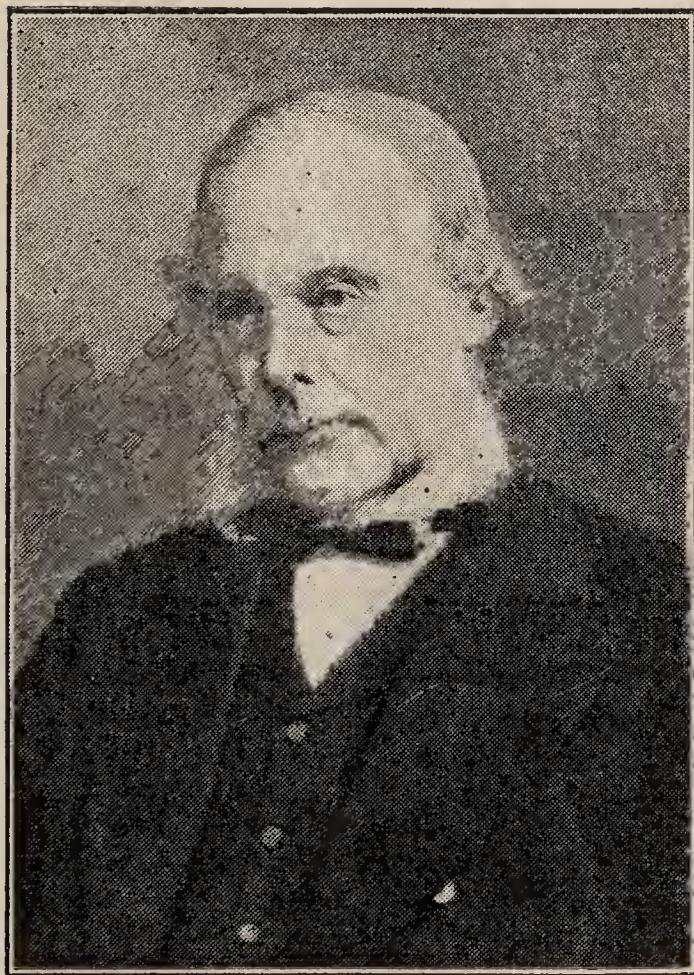
HON. SEC.—S. PAGET, F.R.C.S.

BORN 1827.

LISTER.

DIED 1912.

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the "English Mechanic and World of Science," Jan., 1916).



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We were too busy last year to be thinking of anniversaries. It was five hundred years, in 1915, since Agincourt; a hundred years since Waterloo; and half a hundred years since Lister, in 1865, first ventured to treat the wound of a compound fracture

with crude undiluted carbolic acid. That was the beginning of "modern surgery," which saves, year in year out, all the world over, a thousandfold more lives and limbs than were lost at Agincourt and Waterloo.

Pasteur and Lister—we have always to think of them together, and can hardly think of them apart. They had many gifts in common: the same diligence, humility, simplicity of life, incessant self-judgment, absolute truthfulness, and resolute will to help suffering humanity. In the circumstances of early life, they were unlike. Pasteur was the son of a tanner, who had served under Napoleon; he was brought up in a little French country town, with few "advantages"; he learned to believe in the Catholic Church, in France, and in the glorious tradition of the French Army. Lister was the son of a prosperous London merchant, who also was a man of science, known to all microscopists as the maker of the achromatic lens. He was born and brought up in a pleasant country house—country then, but London now—Upton Manor, where now is Upton Park, E. His people were of the Society of Friends. In the later years he kept the distinctive note of the earlier years, the quietude of the inner life, and he even kept, in his letters to his father, the use of "thee" and "thou." Home gave him that good blend of quiet religion with steady intellectual culture which is the rule in "the best Quaker families"; and he enjoyed all the advantages of a high level of education, interests, and friendships.

Thus unlike in their bringing up, Pasteur and Lister were unlike also in what they had to endure in the middle years of life. Pasteur, between 1865 and 1870, had to bear, as best he could, the burden of illness, of threefold bereavement, and of the agony of the Franco-Prussian War. Lister had no such tragedy. Up to 1893, when the death of Lady Lister—they had no children—left him without much to live for, he never wanted for happiness; he had his share of anxieties and difficulties, but no time of profound misery.

This difference between the two men was written in their faces; and I have often wished that we could have a portrait of Pasteur by Rembrandt, and a portrait of Lister by Raeburn. The Rembrandt portrait would have given us the heavy sombre look of Pasteur's face, the intentness of the eyes, the touch of passion under restraint, the signs of grief and of ill-health. The Raeburn portrait—but no artist could put on canvas a face so sensitive as Lister's. In "Hospital Idylls," Henley has described it well; he was one of Lister's patients in the old Edinburgh Infirmary. It was a face delightful to watch, but hard to interpret. Part of its charm was the delicate, clear pink-and-white complexion; part was the gentle dignity, and the air of thoughtful kindness. It was not what people call a very strong face, not vivid, nor sharply marked; it did not look so purposeful or masterful as he was. It was the face of a very good man, steadily wise, and at peace with himself; the curves of the mouth soft and subtle, the

eyes rather dreamy. He smiled, not laughed; and he could express more by smiling than most of us can express by laughing. He was not often angry; and I cannot imagine him restless, or pompous, or theatrical. He bore himself toward his juniors and inferiors with rather elaborate "old-fashioned" courtesy, which sometimes bewildered them. Often, in the later years, he looked tired, and so he was, doubtless; tired of work, honours, praises, tired even of the world's gratitude.

He was born in 1827; he went to a good private school, then was a student at University College Hospital, and later was a house surgeon there. Among his teachers were Graham, the chemist, and Sharpey, the famous physiologist. From University College he went to Edinburgh to study under Syme, the great Edinburgh surgeon. Later he became Syme's assistant in private practice, and his son-in-law. For many years Edinburgh was his home; he was appointed an assistant surgeon at the Edinburgh Infirmary, and an extra-mural lecturer on surgery. In 1860 he left Edinburgh for Glasgow, to be Professor of Surgery in Glasgow University, and a surgeon at the Glasgow Infirmary. It was at Glasgow, in 1865, that he first employed his method of antiseptic surgery.

We think of him, rightly enough, as a famous surgeon; but he was also a great man of science. Even at University College, he had written some short scientific essays of real original value. At Edinburgh, he laboured incessantly over the scientific problems of the blood, and of the causes of inflammation. His Edinburgh surgical lectures were founded and built on hard and continual microscope work and experimental work. His paper on "The Early Stages of Inflammation" (Phil. Trans. Roy. Soc. 1858) and his Croonian lecture on "The Coagulation of the Blood" (1863) placed him high among men of science. He was a Fellow of the Royal Society at the very early age of thirty-two. If the discovery of the antiseptic method had been nothing more than a happy guess made by a "practical man"—if it had been merely a lucky shot, a sudden idea: "Hang it all! Let's try something else, not these old things"—it might not have stood its ground. It was founded in science, worked-out in science, proved in science.

There is no room here to write about the older theories of infection, and the older ways of dealing with wounds. You can find a sort of prophetic talk about germs so far back as Varro, a Roman writer who lived I know not when. You can find the use of antiseptics so far back as our Lord's parable of the Good Samaritan, the pouring of oil and wine into a wound. This use of alcohol as an antiseptic was enjoined by Hippocrates, centuries before our Lord's time. We must begin somewhere. Let us begin with Semmelweis. He, in Vienna, in 1847, discovered and proved that puerperal fever, and other forms of surgical infection, did not "come of themselves," but were conveyed, by non-sterilised hands or instruments, from patient to patient. By enforcing the strict use of antiseptics, he brought down the ghastly mortality

from puerperal fever, in certain wards of the Vienna hospital, from 12 per cent. to 1 per cent. Unhappily for the world, he could not demonstrate to his critics the actual cause of the disease—the germs themselves. For want of this visible proof of his theory he failed to beat down his opponents, the “doctors of the old school”; and much of his work was lost, and had to be done all over again after his death. That was the tragedy of Semmelweis: he came before his time; he was thwarted, bullied, turned out of office by the older men whose methods he denounced.

Then came Pasteur’s work. Take 1864 as the date of Pasteur’s final public statement of the “germ-theory.” It was on April 7, 1864, that he gave his memorable lecture to all Paris, demonstrating, in flasks of broth, the true nature of all fermentation, decomposition, and putrefaction: proving, once and for all, that infection is due to germs, and that if you can keep germs out of a flask of broth it will not go bad. You need not exclude the air; for it is not the air which sets up putrefaction. But you must exclude the germs, the living dust of the air, for it is these germs which set up putrefaction. You can exclude them by filtering the air, or you can kill them by heating the air. One way or the other, so long as you keep them out of the broth or milk or blood or urine, or whatever organic fluid you may be working with, it will not go bad.

Note, that all men of science, half a century ago, were studying, first and foremost, the presence of germs *in the air*. Not that they overlooked the fact that germs are *everywhere*—on our hands, on our clothes, on everything that we touch, everywhere. But the germs in the air, the “living dust of the air,” especially claimed their attention. And this discovery, that fermentation, decomposition, and putrefaction were due not to the oxygen of the air but to the germs in the air, appealed to surgeons more than to any other men. For they were in terror of the air; above all, they were in terror of “hospital air”; and so were the patients—and no wonder.

I am not going to say much about surgery as it was in the days before Lister. It was, indeed, a most unhappy art, in comparison with what it is now; but it was not always so black as it is painted. A hundred years ago, in all great cities, there was a very terrible period of heavy infection of surgical wounds, and this bad period lasted right up to Lister’s time. The reform of hospital sanitation and of hospital nursing began with Miss Nightingale and Edmund Parkes; but our country had to wait till the Crimean War (1854-56) for Miss Nightingale. The greater number of British surgeons were careful, hard-working, skilful men, keen to do their best for their patients; and some of them were just as wise and observant as our surgeons are now. But till Pasteur had *shown* to men, under the microscope or in a flask, the actual visible germs of infection, the very things themselves, and till Lister had *proved* that wound-infection could be prevented by the scientific use of antiseptics, no great revolution in surgical

methods was possible. Some surgeons were content, more or less, to believe that the fearful death-rate among operation cases was "inevitable"; some few talked as if it were a matter of "chance" or of "luck"; but nine out of ten were longing and striving for clearer knowledge. What *was* infection? It must be something that could hang about a place or hang about a person; something transferable, something divisible, something with a sort of life of its own, and with diversities of character; something in the air, but not only in the air. If only they could see it under the microscope! If only they could isolate and cultivate and observe it, all by itself, outside the human body! Then came Pasteur, and showed them that they could.

During the years 1860-1865 the "germ-theory" was linked-up, more or less closely, with the general way of thinking of infective diseases. Men began to feel fairly sure that these germs were, indeed, what used to be called the *contagium vivum*, or the *materies morbi*; that they were the true cause of diseases, the living agent, the thing itself. But none of the specific germs—such as cholera, plague, typhoid, diphtheria, tubercle—had yet been discovered. The only germs discovered were the "germs of putrefaction," the germs which, if they get into a flask containing broth, milk, urine, blood, or any such organic fluid, make it "go bad." The existence of the specific germs was guessed at, but not proved. Even the "germs of putrefaction," though they had been discovered, had not been strictly classified; nor did men know what we know now, that putrefaction, stench, foulness depend not so much on the germs which start the process of infection as on the subsequent invasion of the organic fluid by further armies of other germs. Thus the phrase "the germs of putrefaction" was more suggestive fifty years ago than it is now.

Still, men's minds were prepared, for some years before Lister's work, to think of germs in association with infection. The address given by Sir Spencer Wells, in the summer of 1864, at the annual meeting in Cambridge of the British Medical Association, comes very near to "Listerism." Nor was the use of antiseptics of itself anything new. Lemaire's use of carbolic acid, for example, was founded, more or less, on the "germ theory"; but it was vague and uncritical in practice. All sorts of antiseptic or disinfectant lotions and washes were being used in surgery; iodine, alcohol, camphorated water, and so forth. Everybody had his own way of "cleansing" wounds already infected past all such cleansing. Poultices, heavily laden with germs, were applied to wounds as bad as themselves. The hands of the surgeons, dressers, and nurses, and the sponges and instruments and towels, were not always clean in the ordinary sense of the word; the arrangements in hospitals for baths and lavatories and pure air were bad; uncertainty, and mere guessing, and false theories about the "intemperature of the weather" and the "unhealthiness of the air" and the "constitutional tendencies of the patient"—these fallacies and errors were wrecking the work of

the surgeons. Skilful operators, brilliant operators—what was the good of their skill and their brilliancy, when so many of their patients died of wound-infection after operation? It was not more skill that was wanted, but more science. And then, at last, in 1865, Lister turned the light of Pasteur's teaching on to the whole problem of wound-infection, and gave to all surgeons a scientific method for the treatment of wounds.

The Royal Glasgow Infirmary, in Lister's time, was part old, part new. Writing in 1870, he speaks of the new part of the building, the "New Surgical Hospital," as a noble structure, with spacious, lofty wards. For all that, it was heavily infected with pyæmia, erysipelas, and hospital gangrene; and in 1867, two years after the founding of the antiseptic method in 1865, excavations made close to the building disclosed a long-forgotten grave-pit, dating from the cholera epidemic of 1847.

For some years before 1865, it had been plain to Lister that the amount of wound-infection was increased (1) by the presence in a ward of many patients with open sores and foul discharges; (2) by the mere overcrowding of a ward with too many patients. As he says in 1870:—

"I was engaged in a perpetual contest with the managing body, who, anxious to provide hospital accommodation for the increasing population of Glasgow, for which the infirmary was by no means adequate, were disposed to introduce additional beds beyond those contemplated in the original construction. It is, I believe, fairly attributable to the firmness of my resistance in this matter that, though my patients suffered from the evils alluded to in a way that was sickening and often heartrending, so as to make me sometimes feel it a questionable privilege to be associated with the institution, yet none of my wards ever assumed the frightful condition which sometimes showed itself in other parts of the building, making it necessary to shut them up entirely for a time."*

Note the phrase, *sickening and often heartrending*: it makes one feel not only the dreadful frequency of wound-infection in the days before Lister, but also the depth of his longing to be of more use. He utterly refused to think of wound-infection as "inevitable," or as "bad luck"; he was compelled, alike by science and by motives of humanity, to work incessantly against it.

Up to 1865, he had done all that could be done by avoidance of over-crowding, by careful cleanliness, by the use of antiseptic washes, and so forth; he had abandoned long ago the old "cold-water dressings": he had tried Polli's use of sulphite of potash as a drug to protect against wound-infection; and he had worked hard for many years, by microscopical and *post-mortem* and experimental research, at the study of inflammation and of pyæmia. Everything was ready for the finishing touch, which the genius of Pasteur gave to Lister.

* "On the effects of the antiseptic system of treatment upon the salubrity of a surgical hospital."—*Lancet*, 1870, vol. i.

In his old age, in 1900, speaking, on a memorable occasion,[†] of the years just before 1865, Lister described his work as follows :

“ While these investigations into the nature of pyæmia were proceeding, I was doing my utmost against that deadly scourge. Professor Polli, of Milan, having recommended the internal administration of sulphite of potash on account of its anti-putrescent properties, I gave that drug a very full trial as a prophylactic. I have notes of a case in 1864, in which, after amputating the thigh for disease of the knee joint, I gave ten grains of the sulphite every two hours from the time of the amputation ; and when, on the sixth day, an ominous rigor occurred, I doubled the frequency of the administration. Death, however, took place nevertheless, and this was by no means my only experience of such disappointment.

“ At the same time, I did my best by local measures to diminish the risk of communicating contagion from one wound to another. I freely used antiseptic washes, and I had on the tables of my wards piles of clean towels to be used for drying my hands and those of my assistants, as I insisted should invariably be done in passing from one dressing to another. But all my efforts proved abortive, as I could hardly wonder when I believed, with chemists generally, that putrefaction was caused by the oxygen of the air.

“ It will thus be seen that I was prepared to welcome Pasteur’s demonstration, that putrefaction, like other true fermentations, is caused by microbes growing in the putrescible substance. § Thus was presented a new problem : not to exclude oxygen from wounds, which was impossible, but to protect them from the living causes of decomposition by means which should disturb the tissues as little as is consistent with the attainment of the essential object.

“ It has been since shown that putrefaction, though a most serious cause of mischief in wounds, is not its only cause. In

[†]The third Huxley Lecture. Delivered before the Medical School of Charing Cross Hospital, October 2, 1900. See Lister’s Collected Papers, vol. ii, p. 515.

§ There is a passage, familiar to all students of Lister’s life, in the very first account which he gave of his discovery of the antiseptic method. In 1867, he published, in the *Lancet*, a paper entitled “ On a New Method of Treating Compound Fracture, Abscess, etc. : With Observations on the Conditions of Suppuration.” In this paper he says :—“ Turning now to the question how the atmosphere produces decomposition of organic substances, we find that a flood of light has been thrown on this most important subject by the philosophic researches of M. Pasteur, who has demonstrated by thoroughly convincing evidence that it is not to its oxygen or any of its gaseous constituents that the air owes this property, but to minute particles suspended in it, which are the germs of various low forms long since revealed by the microscope, and regarded as merely accidental concomitants of putrescence, but now shown by Pasteur to be its essential cause, resolving the complex organic compounds into substances of simpler chemical constitution, just as the yeast plant converts sugar into alcohol and carbonic acid,”

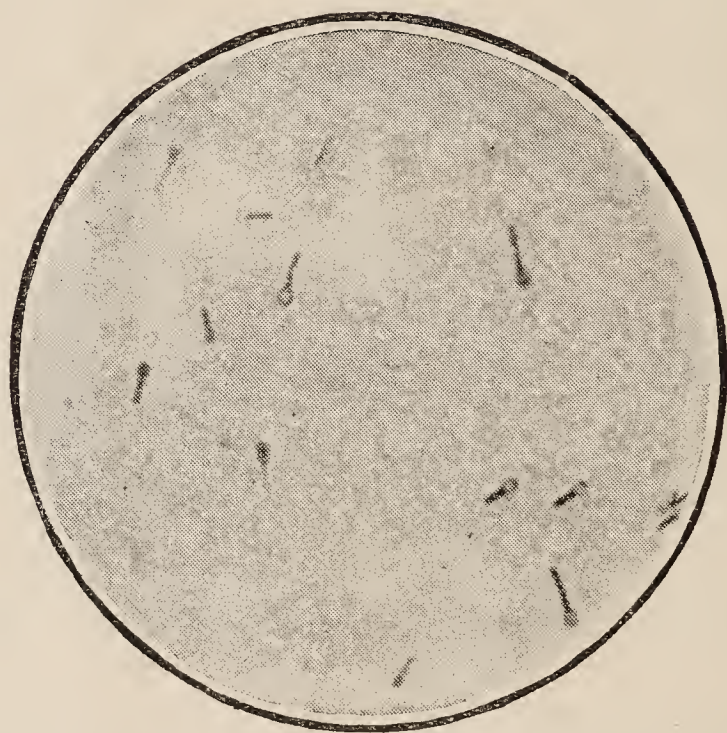
In other words, it has been proved that there are microbes which produce septic effects without occasioning unpleasant smell. But the principle that first guided me still retains, I believe, its full value; and the endeavour to apply that principle so as to ensure the greatest safety with the least attendant disadvantage has been my chief life-work."

To kill those germs which had already got into a wound, and to prevent any more germs from getting in—that was the problem set to Lister half a century ago. He chose for that purpose carbolic acid. The reasons which made him choose it were valid enough half a century ago, but they sound strange now. He refers to them in his first announcement (*Lancet*, 1867) of his discovery:—

"In the course of the year 1864 I was much struck with an account of the remarkable effects produced by carbolic acid upon the sewage of the town of Carlisle, the admixture of a very small



STREPTOCOCCI.



BACILLI OF TETANUS (LOCK-JAW).

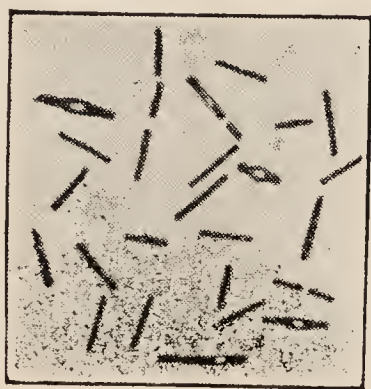
proportion not only preventing all odour from the lands irrigated with the refuse material, but, as it was stated, destroying the entozoa which usually infest cattle fed upon such pastures."

He used at first ordinary "German creosote"; later he used the pure glacial acid, which in 1865 was a new chemical curiosity in our country. His first series of cases were compound fractures and large abscesses. The first case (March, 1865) failed; the second case (August, 1865) succeeded! Very soon he found that he could safely dilute the acid, either with water or with oil. Over the sixth case of compound fracture (June, 1866), a child run over, he worked six months to save the child's leg, and saved it. In 1867 he published his two years' work; the results of the

method were such as the average surgery in hospitals gave no hope of obtaining. Moreover, he had banished from his wards the terror of wound-infection. As he says in his paper at the Dublin meeting of the British Medical Association, in 1867:—

“The two large wards in which most of my cases of accident and of operation are treated were amongst the unhealthiest in the whole surgical division of the Glasgow Royal Infirmary. . . . Since the antiseptic treatment has been brought into full operation, and wounds and abscesses no longer poison the atmosphere with putrid exhalations, my wards, though in other respects under precisely the same circumstances as before, have completely changed their character; so that during the past nine months not a single instance of pyæmia, hospital gangrene, or erysipelas has occurred in them.”

So it began, so it went on. Inch by inch, year after year, he carefully felt his way, incessantly watching every little point of advantage in science or in practice. The use of the carbolised ligature, the catgut ligature, and the chromicised ligature; the disuse of the carbolic spray, which he had invented to put out of action the germs in the air; the use of the cyanide dressing; the endless modifications and adjustments and extensions of his



BACILLI OF “GAS GANGRENE.”

original method—over these he spent his life, working with incessant self-judgment and invincible patience: in Glasgow till 1870, in Edinburgh from 1870 to 1877, in London after 1877.

We all know that his method was criticised, doubted, and opposed by many surgeons in this country, and that there were many years of controversy over “Listerism.” And a good deal of offhand abuse and ridicule has been poured on his critics. But consider the difficulties of this great controversy, as they appeared to men who were in the thick of it:—

1. To believe in Lister they had to believe in Pasteur; they had to accept the “germ-theory” as a final and absolute fact of science—not easy, forty or more years ago, for British surgeons to throw over their ideas and methods because of a theory invented by a foreigner who was not a doctor.

2. To some surgeons, "Listerism" seemed absurdly solemn over little trivial details—not simple, not commonsensical. Some of them practised a sort of "modified Listerism," neglecting this or that detail. The strict "antisepticians" were up in arms against this "modified Listerism"; they insisted on the observance of every jot and tittle of "Lister's own method"; and there was endless controversy, and some very plain speaking.

3. It is certain that some surgeons got good results by methods which were not Lister's method; and the explanation of this fact, and the unravelling of long reports of rival statistics, went on for years.

4. The operation of ovariectomy was made a sort of standard, by which to judge the value of "Listerism." It was not good logic, to single-out one operation in this way: it only increased the difficulties of the controversy.

5. The test period of Lister's work coincided with many other improvements in hospital practice. Better nursing, better house surgeons and students, better drugs, less frequent use of bleeding and of purging, and a more moderate use of stimulants—all these improvements claimed some of the credit which was being assigned wholesale to "Listerism."

6. Lister's earliest method was, of course, imperfect; his slow and incessant remodellings of it were taken by some surgeons as evidence that there was "not much in it, after all"; the use of the carbolic spray especially was criticised till it was at last abandoned; and in a few cases the effects of carbolic acid were harmful or even disastrous to the patient.

7. As time went on it became more and more the rule to prefer things "aseptic" to things "antiseptic," and this development of surgery deprived "Listerism" of that undivided authority which it had in the early years.

These and other delays hindered the universal recognition of Lister's supremacy. What does it matter now? What if surgeons do sterilise instruments and towels and dressings nowadays, not by chemicals but by heat? The principle, either way, is the same. As Sir William Osler has said, the difference between aseptic surgery and antiseptic surgery is the difference between Tweedledum and Tweedledee. Indeed, the two methods are one method; and if I, to-morrow, were to be run over by a motor, and my leg smashed, and all the dust and filth of the road ground into the wound, I should certainly receive both antiseptic and aseptic treatment.

Or think—who is thinking of anything but the War?—think what is done for our men, thanks to what Pasteur and Lister taught to mankind. They have the protective treatment against typhoid fever. They have the protective treatment, immediately after a wound, against tetanus. They carry with them their antiseptic dressing, and their little phial of iodine, for immediate application. The field dressing station, the clearing hospital, the

base hospital, the home hospital, each in turn gives them careful antiseptic and aseptic treatment, to ensure, so far as possible, the healing of wounds already infected. At the great base hospitals, and even at the front in the motor laboratories, bacteriological research goes hand-in-hand with practical surgery. (The three plates illustrate three forms of wound infection which in this War are of profound interest to all of us: (1) *Streptococcus*, the cause of suppuration and of "blood poisoning"; (2) the bacilli of tetanus—lockjaw; (3) the bacilli of spreading gangrene.) Think what it means that the vast majority of our wounded are restored to health, though so many of the wounds are very deep and extensive.

Moreover, it is only one aspect of Lister's work, that lives and limbs are saved, all the world over, which before his time would have been lost. He did more than that. He enabled surgeons to cure, by operation, legions of patients who before his time would have been left to die. And he made hospitals all over the world proof against outbreaks of wound infection: he took away from them the curse of "hospital diseases."

Of his later life—his innumerable honours, his presidency of the Royal Society, his lonely and enfeebled old age—no room is left to write here. Through many years of infirmity, his bodily life slowly flickered out. We are up against that most impenetrable problem, why there should be misery at the journey's end. But his name and his work are everlasting.

NOTE.

It will be of interest to members of the Research Defence Society to read what Lister said of experiments on animals in relation to his own work. He gave evidence, on November 1, 1875, before the first Royal Commission on Vivisection. He was examined by Lord Cardwell, Mr. (Sir John) Erichsen, Lord Winmarleigh, Sir John Carslake, Mr. Huxley, and Mr. Hutton. See Minutes of Evidence, pages 213-219.

Question 4338. (MR. HUXLEY).—I understand that you consider that experimental physiological research has borne, and is likely to bear, very important practical results. Could you enforce that by anything within your own knowledge?

Answer.—Well, perhaps the best way would be to take my own actual experience. The experiments to which I have referred before with reference, in the first instance, to teaching a class, had the effect of giving me a kind of pathological information, without which I believe I could not by any possibility have made my way in the subject of antiseptics; and that subject, I believe, is becoming recognised as one of considerable practical importance. I have often felt that without the basis

which I derived from experiments on the lower animals, I should never have been able to thread my way through the very perplexing and apparently contradictory facts which I met with in the first instance.

Question 4350. (Mr. ERICHSEN).—Am I correct in understanding that in all your various capacities, as practitioner of surgery and as a teacher of surgery, and also as a man who has been foremost in the advance of surgical practice and science, you have found experimentation on living animals necessary? *Answer.*—That has been so.

Question 4351.—And that through the medium of that experimentation, originally commenced possibly, I think I understood you to say, with other views, you were gradually led to the development of that method of treatment which is now known as the antiseptic system? *Answer.*—Yes.

Question 4352.—That in the pursuit of that inquiry you were led to ligature the arteries of some of the larger animals? *Answer.*—Yes.

Question 4353.—Am I right in thinking that it was necessary that you should discover some substance to use as a ligature which did not produce the irritation which is occasioned by an ordinary ligature? *Answer.*—Yes.

Question 4354.—That was an essential part of the business? *Answer.*—It was.

Question 4355.—And that could only be ascertained by experiments on living animals? *Answer.*—Yes, certainly.

Question 4356.—Had you not made experiments on brutes, you would have had to experiment on man; there was no alternative, was there? *Answer.*—There was no alternative.

Question 4357.—You could not discover such a thing as an antiseptic ligature, for example the antiseptic catgut ligature, by any *à priori* reasoning? *Answer.*—It must be tested, by experiment.